# THE JOURNAL OF PHILOSOPHY

# PSYCHOLOGY

AND

# SCIENTIFIC METHODS

JUN 22 1917
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VOL. XIV. NO. 13: JUNE 21, 1917

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PUBLISHED FORTNIGHTLY

THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

All communications to the Editors should be addressed to PROFESSOR FREDERICK J. E. WOODBRIDGE, or Professor WENDELL T. BUSH, Columbia University, New York City.

Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY, Sub-Station 84, New York City. The annual subscription price (twenty-six numbers) is $3.00 and the charge for single numbers is 15 cents. Advertising rates will be forwarded on application.

The Journal of Philosophy, Psychology, and Scientific Methods is owned and published by Frederick J. E. Woodbridge and Wendell T. Bush, at Lancaster, Pa.

*Entered as second class matter, January 19, 1904, at the Post Office at Lancaster, Pa., under Act of Congress of March 3, 1879.*

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Vol. XIV, No. 13. June 21, 1917

THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

# ON SENSIBILITY[1]

## "MIND" REQUIRED TO EXPRESS DIFFERENCE OF LIFE

ONE who has tried to define life, with all its purposefulness and freedom, as a thing residing in and moving through mechanism without ceasing to be part of it, can not be unacquainted with trouble. Yet, this task accomplished as best it might be, he would be innocent indeed who did not know that if he would go on to define mind in the same empirical way the worst of his perplexities lay before him. For to define a thing in an empirical or pragmatical way is so to define it as to leave no doubt what experiments would inform us whether anything corresponding to our definition existed or no. But where minds are concerned we have not yet or only recently grown scrupulous to avoid making hypotheses about them that suggest nothing in the way of an experiment by which our guesses might be tested. We have pretended to know too much of our own, been content to know too little or nothing at all of our fellow's mind, and empirical health is not in us. So that one to whom his own mind is no immediate certainty and his neighbor's no unapproachable eject has trouble in explaining how and why both of them are objects to be experimented upon like trees or houses—and yet very unlike, too, in the kind of experiment they call for.

As these matters are not old and well-worn, but (if I may judge from the criticism previous papers of my own and of other sympathetic writers have called forth) new and even repugnant to prevailing habits of thought, there may be some advantage in beginning the discussion of them where otherwise one might rather have hoped to leave off. A tentative generalization at the outset has often helped me, a reader, and I see no reason why the like should not be vouchsafed by me, a writer, if only it will not be taken with too much finality. But if I start with some very loose reflections on the nature of mind, it will only be to abandon them as quickly as may be and

[1] Paper written for the Philosophical Club of Columbia University. It presupposes the arguments of two earlier ones: "Mind as an Observable Object," this JOURNAL, Vol. VIII., p. 322, and "The Pulse of Life," this JOURNAL, Vol. XI., p. 645.

to devote the body of my paper to one of the most special and technical problems with which an empirical method can be confronted.

It will be remembered that the empirical method of defining mind begins by identifying what you would do to find out whether a being had a mind or not with what you mean by mind, or, as I have sometimes put it, "the criterion of mind constitutes its definition." Now those who have approached the problem of the criterion of mentality fall into two classes: some think they find mind wherever they find life and others attribute mind to only those forms of life they call the higher. Those who hold the first opinion seem to mean not only that life and mind have the same denotation, but also that there is no difference in the connotation of the two terms. If so, the fathers who invented our categories for us were for once lavish in giving us two names for the same thing, whereas it was their niggardly custom to make out with one word for ten thousand things. On the other hand, those who attribute mind to the higher forms of life and not the lower escape the danger of voiding the distinction between life and mind only to fall into another more serious. For I know of none of them whose test of the presence of mind remains clear without becoming structural. But if there is anything in the poet's saying that tired limbs were invented before feather beds and that thirst is older than wroughten cups, it may be our fathers knew a great deal about minds before they knew anything of delicate anatomy.

There is a way, however, of doing justice to both of these historic motives; the denotation of mind may be the same as that of life, and yet the connotation remain quite different. For if life were all of a level, if all things pursued the same end with no difference of skill, I can not see how we should ever have come by the concept of mind or in what way it would have served us if we had. But in our world there are purposeful beings that differ in their aims and there are beings pursuing the same end that differ in the skill with which they work for it. Take just the latter of these classes (for I must postpone the discussion of the former to another occasion); there is no mistaking the need the biologists have felt to account for the different resourcefulness of beings struggling for the common end of self-preservation by introducing the category of mind. Is it not because this being has a mind the other lacks that it can adapt itself and live where the other fails and dies?

It is the observation of a certain difference of lives that has stung the primitive biologist into inventing mind. But, alas, if we ask his modern representative for a definition of this new aspect of life he either falls into mysticism or returns an answer that amounts to saying: Mind is that to have which is to have what accounts for a greater resourcefulness of behavior. Having arrived so far, he feels

that any further pursuit of a problem so lacking in empirical suggestiveness is beneath his dignity as a scientist and he is ready to turn the whole matter over to a certain entity he calls the "metaphysician."

But what if, having learned a lesson from history, we were to reverse all this; what if instead of saying, "this being is more resourceful than that because it has a mind the other lacks," we were to say, "this being has a mind the other lacks because it is more resourceful." Then all we should mean by the various faculties and graded refinements of mind would be the empirical description and measure of these differences in the conduct of life. To be sure, this would mean that if there were no comparison of minds there would be no mind at all, that we should have no reason for attributing mind to anything if we did not give it more or less than some other. But though it would be absurd to say the like of one's keys or purse or medulla, there are many concepts beside that of mind that owe their meaning to relations. Thus many of us after reading Berkeley are content to say that nothing would have any reality at all had it not more or less than some other, and would feel that we had by honest effort struggled out of the metaphysical mood that would express reality in terms of a Lockian substance whose only definition was to be that which makes things real.

To come, then, to a definition (or rather partial definition) of mind. If one being can accomplish a given purpose in $(n+1)$ types of situation, another in but $n$ of these, we shall call that in which the first is better equipped than the second a faculty of mind. I call this definition partial because (1) it makes comparison of means only, not of ends, and (2) it presupposes two minds, the observing and the observed whose relationship is not explained.[2] But I foresee nothing in the later discussion of these matters that will contradict what has so far been said, although if I knew how to say everything at once I should probably want to put each thing a little differently. Lacking this gift, I do not see how any scientific discussion can avoid a certain "dialectic" that asserts only to take back in part.

## Intensity of Sensation Defined

If I turn back to very ancient history to illustrate the idea set forth, I fall first upon Aristotle's distinction between animals and vegetables. With Aristotle organisms were generically alike in purpose and specifically different in resource. The first gain of resourcefulness he noted he set down to the faculties of sensibility and spontaneous motion that raised the animal above the vegetable species in

[2] *Cf.* however, this JOURNAL, Vol. X., pp. 141 ff.

the scale of life. Our modern love of continuity does not encourage us to look upon the acquisition of faculties as so sudden an affair, but we do still regard differences in the delicacy and variety of sensibility as important to the accomplishment of purpose and as evidence of mental superiority. So, too, spontaneity, originality, inventiveness are made measures of intelligence and are taken to be faculties that make for success. This pair, sensibility and spontaneity, might profitably be driven abreast, for I doubt if we find either of them very far apart from the other; but as I must limit myself to one (not to lengthen the discussion) I choose sensibility. Nor do I conceive that I am proposing any different problem if I offer to discuss what it means to have a sensation of a certain quality and a certain intensity.

When the sun rose on Austerlitz, it warmed a puddle and set up chemical reactions in its shallow depths; infusoria stirred; an emperor made a gesture. Inanimate matter "reacted" physically and chemically; animalcules after the manner of their various tropisms; the emperor in the fashion of a Napoleon. Why do we say that of these various reactions, that of the inanimate stuff shows something less than sensibility, that of the Emperor waving on his armies very much more, while just the simplest life there was there shows pure sensibility, nothing more and nothing less?

The answers may well lead up to a definition of sensibility. And I should say: (1) We do not call the physical and chemical reactions sensitive because they are not teleological. (2) We do not call the Emperor's gesture mere sensibility because, though full of purpose, its stimulus was not the sun's light or heat, but the sun and a fair share of the universe. (3) But we do call the behavior of the simple organism a display of sensibility because, viewed as a reaction, it was teleological, and considered as to its stimulus, this stimulus was confined to mechanical changes at and within the surface of its body. The proof of which is that if we imagined all the rest of the cosmos changed, but in such wise as to leave the mechanical situation at the organism's surface unchanged, we should expect the organism to behave in the same way. It has a sensation, not a perception.

Whence this definition of sensibility: Any body that reacts with a purpose we call its own to a change of mechanical conditions within its contours displays sensibility, or has a sensation.

It follows from this that all living beings are sensitive or have sensations, but it follows equally from what went before that we should have no reason for saying so did not different living beings display such differences of sensibility as lead us to recognize sensations of varying quality and of distinguishable intensity. Wherefore it is to the definition of quality and intensity of sensation that our argument would carry us on.

The older empiricism of a Locke or of a Condillac did everything it could to foster the idea that experience began with sensations. And so it does, very possibly; but it takes a most experienced person to tell what is meant by the "simple idea" with which experience begins. What is the quality, what is the intensity of this first sensation? The learning of the ages is little enough of an equipment for the telling. It is only one who knows a great deal about the world "made out of" sensations that knows anything about the "sensations" this world is made out of. Wherefore one can not avoid, nor should any one want to avoid, bringing as much as he can of his knowledge of mathematics and physics to bear on the task of telling what it is that a human baby or maybe a well-grown ameba has when it is said to have a sensation of this or that quality and of greater or less intensity.

To begin with intensity: only one who has developed the concept of intensity in the physical order can tell what it means in the psychical. Now to explain why, if I increase the amplitude of a light wave, I add to its intensity, while, if I alter its wave-length, I change its quality, is a matter that calls for some consideration. The old opposition between quality and quantity can not have any application to a state of affairs in which every term is quantitative. Why does the amplitude series constitute a scale of intensities, differences of wave-length a spectrum of qualities?

If I am not mistaken, the best English into which we can render the Latin *quale* is not *what*, but what *not!* It would seem that we endeavor to throw what differences we can into various scales of intensity and what we can not we leave qualitative, *i. e.*, *not* intensive. Now a scale of intensities has this that is peculiar to it; its different members are distinguished in terms of energy. But where energy differences are not apparent in a physical series, we allow it to remain a scale of qualities. In this way we can understand how it is that two light waves of different amplitude, but constant wave-length, should be regarded as of different intensity, while two light waves of different period, but the same amplitude, should be called qualitatively different.

Having thus defined intensity of stimulus, we are free to inquire what it is in an organism's reaction to stimuli of varying intensity, or to stimuli of the same intensity under varying conditions of sensibility, that leads us to attribute to this organism sensations of different intensity.

The classic model of such an inquiry is Fechner's, the outcome of which was to define the intensity of sensation produced in a given organism by a stimulus of a given intensity in terms of the following empirical data: (1) The intensity of the physical stimulus measured

in a certain physical scale; (2) the intensity of the stimulus just noticeably greater than this; (3) the intensity of the stimulus just noticeable by the organism under the same conditions of sensibility. This definition of Fechner's meets, I conceive, all the formal demands that the traditional meaning of the term "intensity of sensation" imposes, and we need have undertaken no further labors in this field were it not for one unfortunate thing. And that is that the third of our empirical data can not in general be obtained. It could only be obtained for all possible cases if Weber's law could be established for all ratios of the j. n. g. stimulus to the standard, from the lower threshold stimulus to the upper. This is apparently more than can be done by experimental means so far devised, whereupon one may sum up by saying, Fechner has defined something that if it existed would be a sensation of measurable intensity; but no such thing exists.

Thus the empirical method requires us to undertake anew a reduction of vague traditional ideas respecting intensity of sensation to an exact definition that will satisfy them as well as Fechner's did, and at the same time, by avoiding all reference to Weber's law, permit us to find in experience objects corresponding to the thing we have defined. This I have tried to do with results that I should like to submit to your judgment; I found the task difficult indeed, but so fascinating that it is only by the exercise of a self-restraint I hope will be counted unto me for righteousness that I abstain from plunging into a full discussion of the way this defining formula was arrived at. However, I have never found that philosophers in general shared my fondness for getting my thoughts as quickly as possible into mathematical form and keeping them there as long as they would let me. Therefore, I have contented myself with submitting along with this paper the discussion referred to in case any would do me the favor to look at it: it will be enough for the present purpose if I set down the formula quite baldly, only lingering lovingly on some of its more obvious charms and virtues.

$$I_r = \frac{1}{\log \frac{r_+ - r_0}{r - r_0}} \log \frac{r - r_0}{r_\omega - r_0}.$$

That is, the intensity of the sensation due to a stimulus $r$ is a function of exactly the same form as Fechner's and would be identical with his, if $r_0$ were the zero of the physical scale with respect to which Weber's law was supposed to hold and $r_\omega$ were the "threshold stimulus." I have tried to show that any definition of intensity of sensation must have this form; but that we may find a new interpretation for $r_\omega$ and $r_0$ that (1) makes no reference to Weber's law, (2) is indefinitely more liable to be found applicable to experience, and

(3) enables us to determine the value of $I_r$ by observing the reactions of the organism to variations of the stimulus through an indefinitely small range of values.

The data that must be collected experimentally in order that the intensity of a given sensation may be calculated can be illustrated in a particularly simple case. Jennings has described the behavior of the paramecium under the influence of temperature stimuli. Placed in a trough of water kept at a temperature within a certain range, the paramecia will be found evenly distributed throughout the trough. But if one end of the trough be heated and the other cooled beyond the limits of this range, the paramecia will be found gradually to collect in the region of optimum temperature. This phenomenon comes as near as can be to an exhibition of pure sensibility; in the change of temperature of the medium we have a stimulus describable in terms of mechanism; in the motion of the organism, a reaction obviously teleological, and finally, in explaining this reaction we need take into account nothing in the way of stimulus but what happens at the surface of the organism itself.

It is possible then to ask, what is the intensity of the sensation produced in a paramecium by a temperature lying within a certain range? And to this question it is incumbent on us to find an experimental answer. But this is exactly what our definition of intensity enables us to do very simply. For, suppose the organisms evenly distributed at a given temperature; suppose, then, the temperature increased at one end of the trough until the tendency is noticed for motion to take on a common direction, when we may say the organism has "just noticed" the change in its environment. The other data required for our calculation would be gathered by again decreasing the temperature until the original distribution is recovered at a temperature which, if our formula is applicable, will not be identical with that from which we set out, but which we may define as producing a sensation of the *same intensity* under conditions of "over-estimation." Again increasing the temperature under these new conditions until the change is "just noticed," we have one group of data, and repeating the whole experiment for a range of temperatures as narrow as we please we have all the data required for our calculation. Our definition still stands a chance of proving inapplicable, though faint indeed as compared with Fechner's. Should it prove inapplicable we should have to find new interpretations for some of the *r's* of our formula; but if my analysis is correct, we should be confined to a definition of the same form.

"VIRTUAL" BEHAVIOR

I have chosen this very simple example of what an empiricist would mean by attributing a sensation of a certain intensity to a given living being because the behavior on which his attribution rests is here so obvious. But the case is not so simple as to be free from all suggestion of difficulty. Indeed it can be made to yield one question that seems to me of great importance in defining the sort of behavior on which an empiricist bases his judgment that he is dealing with a phenomenon of mind.

"For suppose," a critic may well ask, "suppose you had not carried out the series of experiments on which you based your calculation of the paramecium's sensation, suppose you had not varied the temperature of the medium—then there would have been no behavior on the part of the paramecium for you to observe. Would the organism have been without a sensation of a certain intensity just because you had taken no measures to ascertain what it was? On the other hand if you say it had the sensation when it did not exhibit the behavior, how can you identify sensation with behavior?"

Here, if I am not mistaken, we come upon the reason for regarding sensibility as "passive," having a sensation as a receiving rather than a doing of aught in the world. For the behavior, I would answer, that measures the intensity of our organism's sensation is not necessarily *its* behavior at all, but the behavior of certain classes of beings to which it belongs.

I have already suggested that all studies of teleologically defined beings are statistical.[2] It will surprise no one then if I maintain that the order of facts on which depends my calculus of a paramecium's mental condition is quite analogous to that on which I base my estimate of a man's chance of life at a given moment. This chance of life is just as much the man's private possession as is his purse or his keys or his medulla; but whereas I can determine these latter properties without placing him in various classes to be studied by the method of averages, I can not mean anything by his chance of life save such a calculus of probabilities as actuarial experience justifies. Just so, the greater part of the "behavior" referred to by the empiricists who would define mind in terms of it, is not *actual* behavior on the part of the being to whom this mind belongs, but *virtual* behavior, which means no more than that it is the behavior we should have reason to expect if such and such experiments were tried. But what I have called our reason to expect certain reactions can be but the experience of what actually has happened in other cases: it is a calculus of probabilities based on statistics and may be made as accurate a calculus based on as rich a collection of statistics as our

[2] This JOURNAL, Vol. XI., pp. 645 ff.

practical interest in the case justifies. It is only an old confusion of categories that can make it seem odd that while it is *A's* mind we are speaking of, it is not by examining *A* alone, but by studying *B*, *C*, and *D*, that the answer to our question about *A* can be obtained—and that not an approximate or makeshift answer; but the only kind of answer that makes that kind of question meaningful.

### Quality of Sensation Defined

While I do not propose to let myself off with a discussion of sensibility in its simplest forms only, perhaps it will be better to say what little it is necessary to say respecting the quality of sensation before departing from the domain of the simple. Still clinging, then, to my paramecium, it will be observed that while I was willing to discuss the intensity of its sensation due to a temperature stimulus, I was careful to avoid speaking of this as a sensation of heat. For imagine an organism so simply constituted that although it reacts to stimuli of different quality (light, heat, chemical, electrical) it responds to all of them in the same way. Let us suppose its only way of reacting to any stimulus is to move, would there be any reason for attributing to it different qualities of sensation because it moved in response to different qualities of stimulus? If we had to find a name for the quality of its sensation, would it not be enough to call it a sensation of discomfort whether light, heat, chemical changes, electricity, or whatever else was responsible for its state?

It is only, then, when a subject responds to one quality of stimulus in a way it does not react to another that we have any reason for attributing to it sensations of different quality. As the development of reactions specific to different qualities of stimulus goes hand in hand with the development of specific sense organs we are only too likely to fall back on a structural definition of the qualitative differences of sensation. The sensation that results from stimulating the eye has the quality of color, while what comes from stimulating the ear has the quality of sound. *À la bonne heure;* but a man who reacts in the same way to a stimulus of red and to one of green is easily recognized to be color-blind long before we know anything about the structural defect in his retina that goes with this infirmity. In short, it is the difference in reaction to different qualities of stimulus that defines qualitative differences of sensation: what relation this difference bears to structure is a secondary question.

Finally, it is evident from our definition of quality as the what *not,* increase in the variety and richness of experience is constantly changing the meaning of each quality as it is differentiated from more and more others. Whence nothing could be more misleading than the account our older empiricism offers of the way in which ex-

perience beginning with simple ideas (or as Helmholtz called them *qualia*) leaves these unmodified and only learns to recognize new combinations of them. It can be no truer that I begin with *qualia* and come by a knowledge of the world, than that I begin with a world and come by a knowledge of the *qualia*.

### The Higher Forms of Sensibility

I have, I see, allowed myself but scant space in which to discuss the phenomena of sensibility in the higher forms of life; but fortunately little more is needed than to adapt the principles already laid down to somewhat changed conditions. As long as I confined myself to those very simple forms of life, the exciting cause of whose behavior lay in their immediate surroundings and was susceptible of a purely mechanical description, it was not hard to tell what behavior I meant when I defined their conscious life in terms of their actual and virtual behavior. But as we ascend in the scale of life the stimulus to actual behavior becomes more and more complex and more and more of the mechanical conditions at the surface of the subject's body may change without altering the trend of his present conduct; while a larger and larger share of the universe remote from the subject can not be conceived different and his conduct remain the same. Thus as I sit writing, the blue of the sky out of the window, the crackling of the logs on the hearth, are of so little importance to what I have on hand that the sky might lose much of its light and the fire die down to silence and yet no change in my behavior be noted. Yet it is not denied that I do receive sensations of some sort from these and a thousand other things in and around me, so that the impression is deepened that to have a sensation is a very passive sort of experience.

Nor, being rather a lover of old terms than an innovator, do I see why sensations should not even from the point of view of the kind of empiricism I have been arguing for continue to be spoken of as passively received impressions of a certain quality and of a certain intensity. But I should interpret this to mean that the behavior one appeals to to define mind in its various aspects must of course include the behavior that doesn't take place as well as the behavior that does. What indeed is meant by asserting that the sky's blue is less intense and qualitatively vaguer to me than to my artist neighbor who is trying to paint it? Is it not that a change in this intensity or in this quality which would make him reach for a new color would leave me scribbling with the same pen? But some degree of change would disturb me too; if there were no way of estimating this virtual behavior of mine (as there would not be if I were blind) I should not be credited with a sensation of light. After all, sensibility in man

is very much like sensibility in the paramecium and I hardly regret that I have left myself so little space in which to deal with it.

## The Inner Mental Life

Having come so far on the way to a definition of sensation, its quality and intensity, I was sensible the way had been dry and if I was not to overtax the patience of even such tried travelers as those who had invited me to lead them for once, it was time I hastened on to some sort of a stopping place. Therefore with a brief reference to the sensory material of dream life as presenting a most obvious case in which neither the stimulus nor the reaction in terms of which I had defined the quality and intensity of sensation was open to inspection, I left my argument to the judgment of those for whom half a word is more than enough. But as it has seemed to very distinguished writers (such as Henri Bergson) that the recognition of sensation as existing where neither stimulus nor reaction is observable to the onlooker was an altogether convincing argument against going to work on the definition of sensation in the way I have, I ought, I think, to take a more leisurely survey of this field and to devote a separate article to a consideration of the inner mental life.

## Supplement

### *Definition of Intensity of Sensation*

If $r$ is a stimulus measured in some physical scale of intensity, and if $f(r_1, r_2)$ is the difference of intensity produced by the stimuli $r_1$ and $r_2$, respectively, then

*Post. 1.*—$f(r_1, r_2)+f(r_2, r_3)=f(r_1, r_3)$

which has for its solution

$$f(r_1, r_2)=\phi(r_1)-\phi(r_2).$$

If $\phi(r)$ is to have the properties traditionally given to the intensity of a sensation produced by the stimulus $r$, then

*Post. 2.*—The intensity of the sensation produced by stimulus $r$ is independent of the *zero* of the physical scale in which $r$ is measured. Wherefore

$$\phi(r)=\phi(r_\alpha - r_\beta).$$

*Post. 3.*—The intensity of the sensation produced by stimulus $r$ is independent of the *unit* of the physical scale in which $r$ is measured. Wherefore

$$\varphi(r) = \varphi\left(\frac{r_\alpha - r_\beta}{r_\gamma - r_\delta}\right).$$

To interpret $r_\alpha$, $r_\beta$, *etc.*, empirically, let the place of $r_\alpha$ be taken by the stimuli $r_1$, $r_2$, *etc.*, and let $r_{1+}$, $r_{2+}$, *etc.*, be their just noticeably greater stimuli. Let $r_\beta = r_\delta = r_0$, in which $r_0$ is some zero of the physical scale in which $r$ is measured, such that the equation holds

$$\frac{r_{1+} - r_0}{r_1 - r_0} = \frac{r_{2+} - r_0}{r_2 - r_0}.$$

If Weber's law held and if we imagined $r_1$ to remain constant while $r_2$ varied, $r_0$ would have a constant value expressed by the equation

$$r_0 = \frac{r_1 r_{2+} - r_{1+} r_2}{r_1 + r_{2+} - r_{1+} - r_2}.$$

But it is less of an assumption to suppose, not that $r_0$ remains constant under these conditions, but that it approaches a limit as $r_2$ approaches $r_1$. In place of Weber's law we take, then,

*Post. 4.*—

$$r_0 = \lim_{r_2 = r_1} \left( \frac{r_1 r_{2+} - r_{1+} r_2}{r_1 + r_{2+} - r_{1+} - r_2} \right),$$

and substituting the $r$'s so far defined, we have

$$\varphi(r) = \varphi\left( \frac{r - r_0}{r_\gamma - r_0} \right).$$

Interpreting $r_\gamma$ to be the intensity of the stimulus that produces a sensation of zero intensity and writing $r_\gamma = r_\omega$, we may set down

*Post. 5.*—

$$\varphi\left( \frac{r_\omega - r_0}{r_\omega - r_0} \right) = 0,$$

and substituting $r_\omega$ for $r_\gamma$, we have

$$\varphi(r) = \varphi\left( \frac{r - r_0}{r_\omega - r_0} \right).$$

*Post. 6.*—The intensity of the sensation produced by a changing stimulus is a continuous function of that stimulus.

Therefore, since our data are experimental with a probable error attached we may always find an analytic function of $r$ lying within the limits of p. e. that will satisfy the conditions imposed upon $\phi(r)$.

*Post. 7.*—Subjective conditions (of sensibility) remaining constant, the intensity of sensation increases with the stimulus.

*Post. 8.*—Subjective conditions remaining constant the *rate* at which the intensity of sensation increases with the stimulus is independent of what we take to be the sensation of zero intensity. Since

$r_\omega$ is defined as the stimulus producing a sensation of zero intensity, Post. 8 amounts to saying

$$\frac{\partial}{\partial r_\omega}\left(\frac{\partial}{\partial r}\varphi\left(\frac{r-r_0}{r_\omega-r_0}\right)\right)=0.$$

From the postulates so far laid down, we have for $\phi(r)$ the unique solution

$$\varphi(r)=a\log\frac{r-r_0}{r_\omega-r_0}.$$

But our formula has as yet failed to make explicit those "subjective conditions" of sensibility that have been traditionally accepted as playing a part in the intensity of sensation produced by a given stimulus. As these have been made to depend on the j. n. g. stimulus $r_+$, we must determine how this $r_+$ enters into our expression. To do this consider the case in which a unit difference of intensity exists between $\phi(r_2)$ and $\phi(r_1)$, $(r_2>r_1)$, subjective conditions remaining constant. That is,

$$a\log\frac{r_2-r_0}{r_\omega-r_0}-a\log\frac{r_1-r_0}{r_\omega-r_0}=1,$$

in which case

$$a=\frac{1}{\log\dfrac{r_2-r_0}{r_1-r_0}}.$$

Here the only arbitrary quantity is $r_2$, so that if the j. n. g. stimulus is to appear in our formula at all it must be included in our definition of $r_2$. Whence

*Post. 9.*—$r_2=f(r_{1+})$.

But since from Post. 2, the $r$'s of our formula can only enter in the form $r_a-r_\omega$, it follows that

$$f(r_{1+})=r_{1+}$$

and if we now designate by the symbol $I_r$, the intensity of the sensation produced by stimulus $r$, we have the definition of $I_r$ in the form

$$I_r=\frac{1}{\log\dfrac{r_+-r_0}{r-r_0}}\cdot\log\frac{r-r_0}{r_\omega-r_0}.$$

There only remains to be selected the empirical meaning of $r_\omega$, Fechner took $r_\omega$ to be the threshold stimulus and so made the applicability of his definition to hang on the truth of Weber's law. A phenomenon much better established by experience is that of the

over-estimation of the second stimulus. Let $r'$ be the stimulus that under conditions of over-estimation is judged equal to $r$; $r_+'$ the stimulus that under the same conditions is just noticeably greater than $r'$ and $r_0'$ the analogue $r_0$. We shall define a particular case of zero difference of intensity by

*Post. 10.*— $$I_r - I_r' = 0.$$

We may now solve this equation for $r$. The solution is particularly simple when $r_0' = r_0$, and as these may be anticipated to be very close-lying quantities, we might for practical purposes identify them with their mean $\bar{r}_0$.

Writing for brevity

$$\frac{r_+ - \bar{r}_0}{r - \bar{r}_0} = k$$

we readily obtain

$$I_r = \left(\log \frac{k}{k'}\right)^{-1} \cdot \log \frac{r - \bar{r}_0}{r' - \bar{r}_0}.$$

The empirical question of the truth of Weber's law depends upon our ability to find conditions under which the subjective parameters $k$, $r_0$, $r_\omega$ remain constant. This is only a special case of the more general experimental problem, How do these parameters vary with the conditions of the experiment in which $r$ is made to vary?

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## THE ISSUES CONCERNING MATERIAL IMPLICATION

IN a recent article in this JOURNAL,[1] Mr. Norbert Wiener objects to criticisms which I have made of the relation of material implication as the basis of inference. Mr. Wiener's paper fails to join the significant issue, but this may be as much my fault as his. If others have understood my papers as he seems to have done, then in justice to myself, as well as to those who are interested in Mr. Russell's logic, I should attempt some restatement. And the spirit of Mr. Wiener's criticism is so eminently just and fair that I take pleasure in pointing out my agreement with many of his contentions. For the sake of brevity and clearness, the matters to be considered may be summarized in set theses.

1. The relation of material implication, $p \supset q$, which figures in the mathematical logic in *Principia Mathematica*, is not the relation which we ordinarily have in mind when we say that $q$ can be inferred from $p$.

[1] Vol. XIII., p. 656.

2. There is no sound reason why we should abandon our usual meaning of "implies" for this unusual one. The difference between material implication and ordinary "inference" does not point to any inadvertance or shortcoming of our logical sense; it points rather to the novelty—as an "implication"—of the relation $p \supset q$.

3. The unusual character of this relation is responsible for the presence, in any system of symbolic logic which uses it, of certain "peculiar" theorems, *e. g.*, $\sim p \supset (p \supset q)$, "A false proposition implies any proposition," and $\sim(p \supset q) \supset p$, "If $p$ does not imply $q$, then $p$ is true."[2]

4. Theorems (and postulates) in a calculus of propositions are used in two ways within the system itself; (*a*) as premises *from* which further theorems are derived; (*b*) as rules, or principles of inference, *by* which theorems are derived. In any system of material implication, the theorems which we have spoken of as "peculiar" admit of valid use as premises. But they can not profitably be used as rules of inference, either within or outside of the system.

This thesis needs elucidation. The theorems in question *can be* used as rules of inference, and there is nothing in the nature of the system of material implication, as developed in *Principia Mathematica,* to prevent their being so used.[3] But their use in this way gives such propositions as $p \supset (q \supset q)$, "If $p$ is true, then $q$ implies $q$," and $\sim[(pq) \supset q] \supset q$, "If '$p$ and $q$ are both true' does not imply $q$, then $q$ is true." When used as rules of inference outside the system, these "peculiar" theorems give such propositions as, "To-day is Monday implies $2+2=4$," and "Socrates was a solar myth, implies that to-day is Monday." It is characteristic of the propositions so obtained that they assert a "logical" relation between statements which are likely to be irrelevant. Very few of the "peculiar" theorems are even printed in *Principia Mathematica,*[4] although any number of them can be derived from the postulates. Their presence in the system does not, in practise, affect the proofs in later sections of that work.

5. The system of "strict implication" which has been presented

[2] These theorems are the result of using the "principle of addition," $p \supset (p > q)$, and the definition $(p \supset q) = (\sim p \vee q)$, in the same proof. Whenever this procedure produces a theorem which can not be otherwise proved, this theorem belongs to the class I speak of as "peculiar."

[3] The method by which a theorem, or postulate, is used as a principle of inference in *Principia Mathematica* is as follows: The proposition in question must have as its main, or asserted, relation, an implication, $\supset$. Other symbolic propositions are substituted for $p$, or $q$, *etc.*, wherever $p$, or $q$, *etc.*, appear in this proposition, and in such wise that what precedes the asserted $\supset$ becomes identical with some previous proposition. What *follows* this $\supset$, is then separately asserted as a new lemma or theorem.

[4] See *2·02, *2·21, *2·24, *2·5, *2·51, *2·52, *2·521, and *3·4.

in my papers is such that none of the "peculiar" theorems of material implication can be proved from its postulates, but all the *other* theorems of material implication can be so proved (supposing, of course, each material implication replaced by the corresponding strict implication).[5]

6. Therefore, a distinguishing mark of material implication as a species in the genus "possible systems of symbolic logic" is the inclusion of these "peculiar" theorems.

7. By neglecting ever to use these theorems as rules of inference, one may find the system of material implication a sufficiently workable organon of proof, because in making this convenient omission, one uses precisely those principles which are *not* peculiar to material implication, but conform to the ordinary meaning of "implies."

The above theses are sufficient to establish the fact that the system of material implication is defective, in this sense: it contains any number of theorems—theorems about "implication"—which can never be of the slightest value as principles of inference. And it is precisely by the inclusion of these useless theorems that it differs from another, equally consistent, system of symbolic logic. These are the more important considerations; and if I read Mr. Wiener correctly, he does not deny anything so far stated, though he might desire to offer extenuation for what I have called the "defect" of material implication.

We now approach more difficult matters, and care should be taken not to "read into" the theses which follow anything which is not stated.

8. The system of material implication, as developed in *Principia Mathematica,* is derived from precisely formulated postulates and definitions, by operations which are clearly stated and rigorously adhered to. In exactly that sense, it is a mathematically consistent system.

9. The assumptions are true of *their own* meaning of "implies," and the theorems are true in the same sense.

10. The assumptions and theorems are such that whoever grants the truth of the assumptions can not consistently deny the truth of the theorems. The theorems are relevant, in content, to the postulates: the theorems ("peculiar" ones not excepted) *can be,* each and every one, inferred from the postulates.[6]

Mr. Wiener seems to think that I deny some one or more of the last three theses,—I am not sure which ones.

[5] This statement is not quite accurate, because, in developing the system of "strict implication," I made an error which Mr. Wiener does not note. But that error is easily corrected.

[6] See "The Matrix Algebra for Implications," this JOURNAL, Vol. XI., esp. p. 598.

11. It *is not demonstrated* in *Principia Mathematica* that the theorems of the system of material implication can be inferred from the postulates.

Mr. Wiener devotes the larger part of his paper to the discussion of this thesis. Unfortunately, he seems to confuse "$p$ can be inferred from $q$" with "$p$ *is* inferred from $q$," and with "It is *demonstrated that* $p$ can be inferred from $p$."[7] If every proposition which can be inferred from another were, by that fact alone, demonstrated to be so inferrable, then "proof" would be unnecessary. Also, he seems inclined to fall back upon the assertion that the theorems are derived from the postulates by (mathematically or symbolically) rigorous operations.[8] This would be to beg the question whether a derivation based upon material implication is, or can be, inference.

The *pros* and *cons* of this matter can be presented most succinctly in the form of a dialogue between $X$ and myself. $X$'s sentiments may not be Mr. Wiener's; and any rhetorical advantage which I may take of my straw-man is not intended for him. But I try to state both sides fairly: the main object is elucidation of the issue. At best, this is not easy, and the reader should have constantly in mind the character of material implication. "$p$ materially implies $q$" means exactly: "At least one of the two propositions, '$p$ is false' and '$q$ is true,' is a true proposition;" or "It is false that $p$ is true and $q$ false." If $p$ is false, then $p$ materially implies $q$; if $q$ is true, then $p$ materially implies $q$. Of any two true propositions—however irrelevant in content—each materially implies the other. Of any two false propositions, each materially implies the other. Of any two propositions one of which is true and the other false, the false one materially implies the true one. Of any two propositions chosen at random, it is certain in advance that at least one of them materially implies the other.

*X.* It is demonstrated in *Principia Mathematica* that if the postulates of the system of material implication are true, then the theorems are true.

*L.* Really, you beg the question with your "if . . ., then . . .," which may mean a connection of the kind we call "logically necessary" between the content of the "if" clause and the "then" clause,

[7] See his paper, the last sentence beginning on page 658. This is Mr. Wiener's final word on the topics with which we are concerned. The remainder of his article is given over to discussion of certain related matters and incidental criticism of a view which is not mine. He attributes it to me on the basis of one sentence, the context of which he ignores, and a footnote which he has misinterpreted.

[8] On page 657, where one of the two issues is stated to be: "Is the coherence and self-consistence which it [the Russellian logic] claims for itself, and tries to justify by an orderly derivation of its theorems from a small and simple set of postulates, genuine or factitious?"

or may indicate only a relation of material implication, such as, "If to-day is Monday, then $2+2=4$."

*X.* It means a material implication, but that makes no difference to the argument. The nature of material implication is such that if it holds between postulates and theorems and the postulates are true, then the theorems are true. And *that* is the criterion of valid inference.

*L.* I might ask you whether this "if . . ., then . . ." is a material implication, but I refrain. As a fact, it is a strict implication.[9]

*X.* Then you grant my last statement?

*L.* I grant that the postulates of material implication are true, of their own meaning of "implies," and since true propositions do not materially imply false ones, the theorems are true in the same sense. Still I must point out that true postulates may materially imply theorems which are *quite irrelevant.* That is what the "Too Brief Set of Postulates"[10] was written to show. Does that paper prove that a true postulate may materially imply theorems which are true but irrelevant?

*X.* In a way, it does. The assumption in that case is a "postulate" by courtesy only; but I grant that you can write down a true assumption which will materially imply irrelevant propositions. However, that is not the point: the important thing is that material implication can never hold between true postulates and *false* theorems.

*L.* I have said that true propositions do not materially imply false ones.

*X.* Then you haven't a leg left to stand on.

*L.* Wait a bit. Are the postulates of material implication demonstrated in *Principia Mathematica?*

*X.* Of course not; but they are explained so that anybody can see they are true.

*L.* They are true, as I have said, but are they demonstrated?

*X.* No.

*L.* Then in what sense is the truth of the theorems which they materially imply demonstrated?

*X.* In the sense that, whether the postulates are true or not, if you *grant* the postulates, you must grant the theorems.

*L.* I am sorry if I seem to split hairs, but your statement is not sufficiently precise. We "grant" assumptions in two senses,—either because we believe they are true and are ready to believe what they imply, or for the sake of the argument and to discover their logical

[9] The postulates of the "Matrix Algebra" give the theorem, $[(p \supset q)p] \cup q$, where $\supset$ indicates a material implication, but $\cup$ is a strict implication.

[10] This JOURNAL, Vol. XII., p. 523.

consequences. "Postulates" are usually granted in the second sense, are they not? Do you use "grant" in this sense?

*X.* It is not of the smallest consequence.

*L.* It would not be if we were concerned with the ordinary meaning of "implies," but material implication is peculiar in that respect. Let me ask you: Do you mean that, whether the postulates of material implication are, in fact, true or not, if they *were* true, the theorems *would necessarily* be true?

*X.* There is no mysterious physical or causal connection in material implication—or any other kind—if that is what you are driving at.

*L.* But tell me: do you admit that "Socrates was a solar myth" materially implies $2+2=5$?

*X.* Yes; but only because Socrates was *not* a solar myth.

*L.* Quite so. But if Socrates *were* a solar myth, would it be true that $2+2=5$? If you granted some paradoxer his assumption that Socrates was a solar myth, would you feel constrained to go on and grant that $2+2=5$?

*X.* I suppose you mean to harp on "irrelevant" some more.

*L.* I mean precisely this: The postulates of the system of material implication in *Principia Mathematica* are not demonstrated, and the "proofs" of the theorems show only that they are materially implied by the postulates. Suppose, for the moment, the postulates were not true. Then not only might the theorems be false, but—for anything that is *demonstrated*—they might be such that if the postulates *were* true they would *still* be false.

*X.* What of it, since the postulates are, in fact, true?

*L.* Only this: the relation which is established between postulates and theorems, is the relation which *is established,*—whether the postulates are true or not does not alter *that.* And the relation which is established between the postulates and theorems of the system in question is precisely the same relation which you admit to hold between "Socrates was a solar myth" and $2+2=5$. Perhaps I am a purist, but I should never call the establishing of a connection like that "demonstration" or "proof."

*X.* But you know well enough that the postulates and theorems of the system of material implication are relevant to each other.

*L.* Yes, I know that they are, in fact, relevant. As I have said, the theorems can be inferred from the postulates. Once more, the question is not whether they *can be* inferred, but whether this is *demonstrated?* You see the question always comes back to the proper meaning of "implies" which ought to figure in demonstration and proof. "Proof" requires that a connection of content or meaning or logical significations be established. And this is not done for

the postulates and theorems in material implication: what is more, it can not be done without calling on principles outside the system.

*X.* Let me see if I understand you. You grant that the relation of material implication is such, and the procedure of the early sections of *Principia Mathematica* is such, that the postulates being, in fact, true, and the fact that they materially imply the theorems being established, it follows that the theorems are true?

*L.* Yes.

*X.* And when you say that the theorems are not demonstrated, all you mean is that they are not shown to be relevant in content to the postulates?

*L.* Precisely that. You think that this is a very little "all"; I think it is a big "all." For a relation which does not indicate relevance of content is merely a connection of "truth values," not what we mean by a "logical" relation or "inference."

It is regrettable if my use of "demonstration" and "proof" has confused any one, but I think I have used the terms in their commonly accepted and proper signification.

And now I think the matter is concluded, fairly to all concerned.

*X.* But Mr. Russell has a new and better meaning of "demonstration." You do not even mention that. In the later sections—

*L.* May I not make my offering to the gods without reciting the whole *Principia Mathematica*?

And should we speak of new and better meanings, or of new and better analyses? "Implication" and "proof" are pretty fundamental categories. The contributions of Mr. Russell and Mr. Whitehead are important—none more so—and yet they will not thank us if we speak as though logic and mathematical demonstration were things which they invented and can do with as they please. We are always in need of clearing away confusions and realizing more clearly our own intentions. But where the basic things of human reason are concerned, genuinely novel meanings are justly regarded with suspicion.

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## REVIEWS AND ABSTRACTS OF LITERATURE

*The Human Worth of Rigorous Thinking.* C. J. KEYSER. New York: Columbia University Press. 1916. Pp. 314.

This book is a collection of essays and addresses which have appeared in various periodicals during the last fifteen years. They concern a variety of topics, ranging from issues of such a metaphysical character as that of "The Existence of the Hypercosmic," and

mathematico-logical questions, such as are exemplified in a paper "Concerning Multiple Interpretations of Postulate Systems and the Existence of Hyperspace," to general educational and cultural problems about "The Human Worth of Rigorous Thinking," "Research in American Universities," *etc.* Although the author claims in his preface that all the separate papers in the volume are concerned with the central question indicated by the title, the bond of union between them is very loose. In so far as the book possesses any single focal idea, it is this: mathematics has its origin in the fact that the intellect, "instead of endlessly and stupidly besieging the illogical world of sensible magnitudes with the machinery of logic, . . . has created for itself another world" (p. 21)—the world of pure mathematics, which may resemble the world of sense, but is not constrained by it. Professor Keyser deduces the irrationality of the world of sense from the existence of sensory limina and the consequent intransitive character of the relations of equality given by the senses. However, there is nothing self-contradictory or irrational about intransitivity. The relation of paternity is not a bit less comprehensible because the father of my father is not my father. Why should the fact that two weights, each of which is indistinguishable from a third weight, may be distinguishable from each other, cause any greater perplexity? The only contradiction to be found in the intransitive nature of the relation of indistinguishability arises from the mistaken and gratuitous assumption that what we perceive when we are unable to discriminate between, say, two weights, is that they possess some common quality, which may be called their weight. It is perfectly possible to give a consistent logical analysis of the relation of indiscriminability, even though it is intransitive. Of course, it is quite true that the reference of our intransitive experience of indiscriminability to a base-system that possesses the ordinary properties which we are accustomed to attribute to measurement, greatly facilitates the interpretation and the correlation of the facts of sense; but it involves merely the substitution of a simpler order for a more complex one, not the creation of Order out of whole cloth. Mathematics has its origin in any logical use of the intellect whatever, and if the intellect chooses to "besiege the world of sensible magnitudes with the machinery of logic," it is there that mathematics begins.

In Chapter IV., which is entitled "The Walls of the World: or Concerning the Figure and Dimensions of the Universe of Space," Professor Keyser discusses Pascal's definition of the Universe, *i. e.*, space as "an infinite sphere whose center is everywhere and whose periphery is nowhere." He shows that this is literally true, or that it is at any rate a precise formulation of our usual beliefs concerning space. He then suggests that it might be possible to form a space

infinitely larger than our ordinary space by considering a sphere whose radius is a line, the length of which is measured by the number of the continuum. This is most unsatisfactory. The measurement of a line does not primarily consist in its decomposition into an aggregate of unit distances, but rather in its traversal by the end-on-end repetition of a unit vector. In the first place, this makes an integer which measures a distance, a *relation-number*, for the unit vector is repeated end-on-end in some sort of a series. In the second place, unless we make an entire revision of our definition of measurement, this number must either be finite, or the number of a progression, or the number of a regression, or the number of a progression followed by a regression, for these are the only numbers pertaining to series in which every term has both an immediate predecessor and an immediate successor. It is possible to see how the definition of measurement might be altered so as to apply to magnitudes with a transfinite ordinal number, or with other relation-numbers of somewhat similar formal properties, for in a well-ordered series every term has a next successor; but in a dense series like the continuum there can be no operation analogous to end-on-end repetition. As to the question whether the cardinal number of the continuum may not be the *cardinal* number of some well-ordered series of vectors determining a transfinite magnitude, one can only say that this possibility depends on the validity of Zermelo's *Auswahlsprinzip*, which there is no hope of either proving or disproving.

Chapter VI.[1] is entitled "The Universe and Beyond." It is devoted to the thesis that "every universe is a component of an extra-universal" (p. 123). This conclusion is in contradiction with one more recently expressed by Professor Keyser. In his *Science and Religion*, which appeared in 1914, he says: "That there is a universe is a fact we can not escape." The context here makes it clear that Professor Keyser means by "universe" an all-inclusive whole, which can have no "extra-universal." Curiously enough, this latter statement occurs in a proof of the existence of the superrational which adduces the universe itself as an instance of a superrational entity. In the present book, which is in this respect much more in accord with the trend of modern logic, the "extra-universal," which seems to be a near relative of our author's more recent "superrational," is sought for in the world of pure essences, which are exemplified by mathematical systems. In the course of his arguments, Professor Keyser indicates his adherence to the consistency theory of truth, as opposed to the correspondence theory, though he gives no grounds for his choice.

In the next chapter we find an interesting illustration of the

[1] Reprinted from *The Hibbert Journal*, January, 1905.

tremendous changes which have come over the theory of infinite numbers during the last twelve years. The title of this chapter is "The Axiom of Infinity: a New Presupposition of Thought." Here, on grounds which appeared sound enough at the first publication of this paper in 1904, but are completely obsolete to-day, Professor Keyser arrives at conclusions which are now held for other reasons. The thesis is that the existence of infinite number has never been proved strictly, and is at the best but a highly probable hypothesis. The proofs of the existence of the infinite presented by Bolzano and Dedekind are discussed, and it is shown that they are based on the gratuitous assumption that some process or other may be repeated an indefinite number of times. The same claim is also made concerning a proof given by Russell in his *Principles of Mathematics,* but this argument is not discussed at all. Nothing explicit is said of those proofs of the existence of the infinite which are derived from the general theorem that the number of subclasses of a class is always greater than the number of its members, and that therefore the series of the positive integers can be extended as far as we wish, so as to contain $\aleph_0$ terms. It was only when the fallacy of this argument was exhibited by Bertrand Russell in connection with his theory of types that the indemonstrability of the Axiom of Infinity was finally rendered almost certain. Professor Keyser's paper, however, is a perfectly valid refutation of the two arguments for the existence of the infinite against which it is directed.

A passage in this paper which would require a still more thorough remodeling to be satisfactory from the standpoint of the present theory of infinites is the following: "*Any class or assemblage (of elements), if infinite according to his* [*i. e.,* Bolzano's] *definition of the term* [= non-inductive, in the terminology of Whitehead and Russell], *enjoys the property of being equivalent . . . to some proper part of itself*" (p. 151). It has since been shown that this statement depends for its truth on Zermelo's *Auswahlsprinzip,* concerning the correctness of which there is the greatest diverseness of opinion among mathematicians. Few indeed care to entrust any of their important theorems to such a shaky foundation. Although one must remember that we are here dealing with an article which, in its original form, antedates the Zermelo controversy, it would hardly be asking too much to expect that in a book designed for non-mathematicians, which can consequently not presuppose in its readers any acquaintance with the status of the mathematical disputes of the present day, we should find at least a footnote pointing out the revisions which would be needed to render the text rigorous according to the standards of the time.

Professor Keyser claims that it can be established inductively

that the probability of the truth of the Axiom of Infinity is "the highest yet attained" (p. 162). However, there is not a single empirical criterion which would avail us in the least in the distinction between an infinite world and one with 101,000,000,000,000 entities in it. All the observations of science are just as susceptible to an explanation which presupposes an enormous, though finite, world, as they are to an explanation involving a continuous or infinite space. The only inductive method which would tend to prove the infinitude of the world is the Method of Agreement; and it is well known how unreliable this is when not combined with the Method of Difference. The question whether space is finite or infinite is of much the same character as that whether it is Euclidean or non-Euclidean: in each case, it is inconceivable that the question should be settled by inductive evidence, and the choice of which hypothesis we are to use in our scientific investigation is entirely a matter of technical convenience.

In Chapter XIII., entitled "Concerning Multiple Interpretations of Postulate Systems and the 'Existence' of Hyperspace," Professor Keyser tries to show that "hyperspaces have every kind of existence that may be warrantably attributed to the space of ordinary geometry" (p. 256). After first maintaining that the space of our experience is irrational, and hence not a suitable subject-matter for a mathematical science, on grounds the falsity of which has already been exhibited in this review, Professor Keyser proceeds to investigate the nature of the existence which belongs to our ordinary three-dimensional space. He claims that every system satisfying the postulates of geometry must have properties not accounted for in these postulates, and that the properties of the system which is three-dimensional space *par excellence* are extensional in character. "Extension," according to him, is derived from a sort of schematization of the space of our experience. He says that the mind arrives at it by "doing certain things to the sensibles, to the raw material of our mental architecture" (p. 252). It is the particular geometry of this extensional space that he calls pure geometry.

After giving a casual mention to the view that geometry consists, not in a special deductive system in which complete propositions play a part, but in a general form of deduction, which concerns itself with "propositional functions," in which the points, lines, *etc.*, are variables, not constants, Professor Keyser proceeds to discuss four-dimensional space. He holds that this can be obtained by considerng a system with four independent kinds of "extension." It is clear, then, on the basis of what we have already seen, that the assertion that four-dimensional space exists in the sense in which it is

here affirmed, reduces itself to the statement that our spatial experience may in some way be so schematized as to produce a manifold of "points" depending upon four independent measurable quantities. This may be so, but in all Professor Keyser's argument, there is not one iota of evidence to prove it, and it is certainly very far from being immediately obvious. Indeed, it is an hypothesis which almost every one will reject, once it is explicitly stated. As a consequence, the entire thesis of the paper falls to the ground. Four-dimensional geometry certainly exists as a general form of deduction, but there is no reason whatever to assume the existence of four-dimensional *space*.

The remaining articles comprise a very good review of the *Principia Mathematica* of Whitehead and Russell, several interesting pleas for the cultural value of mathematics, and papers on more general educational topics. As a whole, the book is very readable, yet contains much that is worthy of consideration by the serious student, be his interests primarily mathematical or philosophical.

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*Group Theories of Religion and the Religion of the Individual.* CLEMENT C. J. WEBB. London: The Macmillan Company. 1916.

Mr. Webb, who is a fellow of Magdalen College, is best known in this country as the author of the little book on the *History of Philosophy* in the Home University Library, though he has to his credit two much more ambitious volumes of a theological nature. The book under review presents in outline the fundamental views of what might be called the Durkheim school of sociology concerning the nature of religion, and subjects some of them to a rather severe, but probably well-merited criticism. The criticism, though severe, is discriminating, the leader of the school coming off much better than some of his followers. Even so, it is a question whether M. Durkheim does not deserve to fare even better than Mr. Webb permits; for Mr. Webb's book was written (though not published) before the appearance of *Les Formes Elémentaires de la Vie Religieuse*, and some of the positions criticized in the book under review are to be found only in Durkheim's earlier work (in the *Année Sociologique*) and are considerably modified in the later and larger treatise. There is, however, still a good deal left in *Les Formes Elémentaires de la Vie Religieuse* which is open to Mr. Webb's criticism—notably the theory of the social origin of the categories. After analyzing this hypothesis and the facts on which it rests, Mr. Webb concludes that what truth there is in it reduces to the rather mild assertion that the individual gets his point of view from his own group; "what sociology can ex-

plain is not why we use categories—meaning by that word 'principles of classification of universal application'—but why certain particular principles of classification were first hit upon rather than others."

The disparaging view of religion held by many of Durkheim's followers—notably Mauss, Hubert, Cornford, and Miss Harrison—which would reduce religion ultimately to the survival of a primitive "collective representation," is attacked with a good deal of enthusiasm and considerable skill. But the best of his ammunition the author reserves for M. Lévy-Bruhl and his theory of a prelogical mentality incapable of using the principle of contradiction—of which low mental state religion is an unfortunate survival. According to our author it is M. Lévy-Bruhl who is innocent of the principle of contradiction and does not even know what it means. Mr. Webb's own view of religion would reconcile both individualistic and group theories. "Religion can never assent to an individualism which finds the characteristic of individuality in bare exclusion of all that is other than the individual itself, and thus robs the individual itself of all content; since the religious soul knows that only in proportion as what it finds in itself is not its own, but God's, has it anything worth calling its own. But, on the other hand, the religious soul must find this in itself; and if it has no self in which to find it, it can not find it at all. That what has become its own should cease to be its own would mean that having found itself in losing itself, it would then lose itself again, and this time without finding itself at all."

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## JOURNALS AND NEW BOOKS

AMERICAN JOURNAL OF PSYCHOLOGY. January, 1917. *Attitude in Relation to the Psychological Judgment* (pp. 1–37): S. S. GEORGE.—From a series of controlled introspective studies the conclusion is drawn to the effect that an observer can maintain a constant disposition or attitude toward judgment material. Judgments of "greater," "equal" and "less," are the result of a constant attitude while "doubtful," "no-difference" and "or-judgments" represent variations from the constant type. *The Significance of Stimulation in the Development of the Nervous System* (pp. 38–56): WILLIAM H. BURNHAM.—A review of the important studies of factors that develop the central nervous system indicate that stimulation or the functional development is most important. The importance of dynamic physiology in relation to psychology is indicated. *An Analysis of a Phase of the Process of Classifying* (pp. 57–116):

SARA CAROLYN FISHER. – An introspective, analytical study of the classifying process shows that the first step consists in the manner of perceiving. In this perception there arose the consciousness of resemblance or the sense of its absence. When no resemblance was noted the course of attention was retarded. *Research in Pathological Psychology and Biochemistry* (pp. 116–140): EDWARD COWLES. – The development of psychological experimentation along with the physiological and biochemical researches in the American hospitals for the insane is discussed. Psychiatry is laying broader foundations based on dynamic psychology and the new revelations of physiology. *On the Number of Articles of Psychological Interest Published in the Different Languages* (pp. 141–150): SAMUEL W. FERNBERGER. – English psychological interest is increasing while the French is decreasing. Students of psychology should have a ready knowledge of English and German. *Minor Studies from the Psychological Laboratory of Vassar College. A Study of Freshmen* (pp. 151–155): SOPHIE D. WHITE, SYBIL MAY, M. F. WASHBURN. – Several tests on freshmen showed that good results in them is correlated with good work in freshmen classes. *Directed Recall of Pleasant and Unpleasant Experiences* (pp. 155–157): MILDRED F. BAXTER, KOTO YAMADA, M. F. WASHBURN. – Time for recalling unpleasant experiences slightly longer than pleasant. Pleasantness more often considered mental and unpleasantness physical. *Accuracy of Visual Memory and Speed of Verbal Perception in Poor Spellers* (pp. 157–158): ANNETTE HOWELL, LUCILE HOPSON, M. F. WASHBURN. – Good spellers as a group have a more accurate visual memory. *Book Notes* (pp. 159–162).

Arnold, William R. Ephod and Ark: A Study in the Records and Religion of the Ancient Hebrews. Harvard Theological Studies, III. Cambridge: Harvard University Press. 1917. Pp. 170.

Farrell, H. P. An Introduction to Political Philosophy. New York and London: Longmans, Green, and Company. 1917. Pp. ix + 220. $1.25.

Marvin, Walter T. The History of European Philosophy: An Introductory Book. New York: The Macmillan Company. 1917. Pp. xiii + 439.

Smith, Henry Bradford. A Primer of Logic. Pulaski, Va.: H. D. Smith and Brothers. 1917. Pp. 48.

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## NOTES AND NEWS

READERS familiar with Schiller's *Studies in Humanism* will recall his clever *jeux d'esprit* in the way of dialogues purporting to have been held between a disciple of Protagoras and an orthodox thinker

of the fourth century, B. C. Mr. Schiller's cleverness has had an outcome recently which he could hardly have anticipated. Dr. White in his just published book, *Mechanisms of Character Formation,* quotes (without reference to Schiller) from the "Papyri of Philonous" no less than three times, once at great length. This is a tribute to Dr. Schiller's literary skill, but it is also unfortunately indicative of the uncritical way in which it has become fashionable for many Freudian writers to deal with anthropological and literary sources and documents. It would assist the more general acknowledgment of the many strong points in Freudian psychology, if the members of that school could agree upon the selection of a competent censor to pass upon all their anthropological and literary interpretations before they see the light of day.

DR. FELICE FERRERO, American representative of the Italian press, has recently given a series of three lectures on the "History of Science" at Northwestern University. The titles of the individual lectures are as follows: "The Period of Philosophical Speculation: Theories of Evolution and Arithmetic"; "The Shift to Experimental Methods: Aristotle, the Naturalist, the Astronomer, and the Physicist"; "The Great Lights of Ancient Science: Archimedes and Hipparchus."

GEORGE HERBERT PALMER, Alford professor of philosophy in Harvard University, delivered the annual charter day address in the Greek theater of the University of California, the forty-ninth anniversary of the chartering of the university of California by the state.

PROFESSOR EDWARD B. TITCHENER, head of the department of psychology at Cornell University, recently gave a lecture at Princeton University on "The Association of Ideas."

PROFESSOR JOSEPH JASTROW, of the University of Wisconsin, recently gave a series of lectures at the Ohio State University on "Sources of Human Nature."

DR. A. H. SUTHERLAND, instructor in psychology at Yale University, has accepted a position as psychologist in the public-school system of Los Angeles.